The Dodo Bird Is Extinct

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It may be quite inaccurate and certainly it is premature to identify the dodo bird as the mascot for psychotherapy research. The dodo bird is not as alive and well as some may wish. The complexity of determining the presence of specific effects has been underestimated and, more importantly, the evidence of specific effects in treatments have been largely ignored by those who adopt a dodo bird's perspective. The article by Luborsky et al. in this issue illustrates these misperceptions and exclusions. Here I review some of the evidence for the premature ascendence of the dodo bird and raise questions about the nature of current research methodologies for assessing the presence of specific effects and especially for disclosing differential treatment contributors to treatment outcome.

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Not Freud, Reich, Adler, or Luborsky is more widely cited than the lowly dodo bird in psychotherapy research literature. So much do we psychotherapists and psychotherapy researchers admire this bird that we often forget that it does not actually exist. In spite of the best efforts of many in the psychotherapy professions to make it a phoenix that rises from the ashes of extinction, the dodo bird remains extinct. Its resurrection in contemporary research does not warrant its becoming either the mascot of psychotherapy or the flag bearer of psychotherapy research. Nor do the rumors of its being rediscovered through meta-analyses do justice to what is known and what is not known about the value of specific psychotherapy interventions.

Luborsky and colleagues (this issue) remind us that there are ingredients that are common to all treatments that account for a good deal of the observed treatment change. No argument! And if the dodo would stop with this conclusion, all would be fine and good. But, the dodo bird argument takes this simple fact and extends it to (a) a conclusion of equivalence among treatments and (b) the award of prizes to all. Although the classic box-score analysis conducted by Luborsky, Singer, and Luborsky (1975), to which the Dodo bird verdict is most often attributed, demonstrated that there were few differences that significantly distinguished among various brands of psychotherapy, this conclusion was developed and has been maintained by adhering to some uncomfortable (and frequently unquestioned) methods.

Shortly after it was published, a reanalysis of Luborsky et al.'s (1975) findings that disaggregated patients by common aspects of normal personality, problem complexity, and severity (as opposed to diagnoses), and disaggregated treatment into families of procedures (as opposed to therapy schools) uncovered several moderately strong differential effects of treatments (Beutler, 1979). Such findings are ignored, however, because in the search for the dodo, there are rules of engagement that make the extinct bird appear to be alive and well. These rules virtually ensure that differences among patients and treatments will be occluded. Thus, the dodo bird is made to appear by collapsing the 100 or so different types of psychotherapy patients into a single group and by consolidating the 400 or so different types and procedures that constitute psychotherapy into a half-dozen groups. In other words, the dodo bird only appears to be alive when one accepts the myths of patient and treatment uniformity about which Kiesler (1966) warned us nearly four decades ago.

Luborsky et al. (this issue) acknowledge but discharge those who have advocated an inspection of how patients and treatments fit together by incorrectly asserting that the nature of patient mediators of treatment effects have not yet been investigated. On the contrary, a very large number of studies have addressed and supported the presence of patient qualities that mediate and modify the effects of treatments. Although this brief article does not allow a comprehensive review of this literature, the near-religious reverence with which many hold the dodo bird suggests that it may be of value to explore some of the evidence for treatment specificity.

Luborsky et al. (this issue) collapse into one category patients with depression, obsessive-compulsive disorder, phobias, other anxiety disorders, and "mixed neurotics." They also reduce the more than 150 empirically defined treatments (e.g., Chambless & Ollendick, 2001) into five

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psychotherapy groupings and then add an equally nondiscrete "drug treatment" group to the list. By so doing, they have fallen prey to the homogeneity myths and implicitly assumed that both psychiatric outpatients and treatments were functionally equivalent to one another.

IS IT LEGITIMATE TO REDUCE ALL OUTPATIENT VARIABILITY TO A SINGLE GROUP?

By collapsing all outpatients, regardless of personality and psychopathology, into a single group, those who seek to reveal the dodo bird assume at the outset that these various qualities do not differentially determine patients' response to treatment and could not possibly cancel out meaningful effects by undisclosed heterogeneity. In fact, patient-specific responses to different treatments have been observed at two levels. In their qualitative review, Chambless and Ollendick (2001) found convincing evidence that there are differences in how anxiety-disordered and depressed patients respond to different treatments. They concluded that cognitive-behavioral therapy is more effective than nondirective, relationship therapy among patients with generalized anxiety disorder; exposure therapy is more effective than relaxation therapy or anxiety management among those with obsessive-compulsive disorder; and cognitive behavior therapy is more effective than nonspecific and educational therapies among those with panic disorder. Patients with major depression, however, respond similarly to many different therapies. Because the meta-analyses studied by Luborsky and colleagues are heavily loaded with studies of depression, differential effects are doomed to be masked.

Beutler, Clarkin, and Bongar (2000) went beyond a diagnostic analysis and explored state and trait variables that might result in differential treatment responses. First, through a comprehensive review, they uncovered more than 300 studies in which a patient variable exerted a significant mediating effect on treatment—"different strokes for different folks." Most of these studies reflected on a relatively small number of reliably defined factors. Thus, using a multisite sample that studied nine different treatments, they cross-validated six of these dimensions. They found that problem complexity (e.g., dual diagnosis, chronicity), patient interpersonal styles, impairment levels, distress, and coping patterns all evoked a differential response to different qualities of treatment (e.g., modality, format, symptom vs. conflict focus, level of directiveness). More recently, additional comprehensive reviews have added more studies to those that support the value of several of these dimensions (Beutler, Harwood, Alimohamed, & Malik, in press; Beutler, Moleiro, & Talebi, in press).

One might ask why this evidence is not more widely cited, known, and believed. The very nature of patient–treatment matching makes research on patient–therapy matching difficult to conduct (Kazdin & Bass, 1989), and summarizing it is hampered by great variations in the concepts used and levels of analysis performed. However, it may also be that this literature is kept invisible by the unquestioning faith that many hold in the existence of the dodo bird and the associated skepticism that many have for any disconfirming evidence.

IS IT LEGITIMATE TO COLLAPSE TREATMENTS?

It is easy to believe that two treatments that share a common theory and that describe similar treatment processes are functionally equivalent. Thus, on one hand, it is relatively easy to justify collapsing cognitive and cognitive-behavioral treatments. On the other hand, it is more difficult to rationalize the consolidation of such widely different theories as transpersonal, systemic, client-centered, experiential, existential, and so on, under a label of "dynamic," as done by Luborsky et al. (this issue), without feeling that something was sacrificed in the translation. Even the well-known Task Force on the Promotion and Distribution of Psychotherapy Procedures (1995) sponsored by the Division of Clinical Psychology required that, in order for different treatments to be considered functionally equivalent, they must come from the same theoretical model and from the same manualized description.

A dramatic parody of the logical inconsistency of homogenizing theoretically diverse treatments is seen in Luborsky et al.'s (this issue) decision to consider medical treatments as a single entity. The decision to ignore either the type of medication used or the widely diverse effect of these medications on different symptoms displays a remarkable absence of faith in pharmacotherapy. Given the diagnostic heterogeneity of the sample and the variability of contemporary prescriptive practices, it is likely that the actual medications lumped together under the title, "drug treatment" included tricyclic antidepressants, selective serotonin re-uptake inhibitors, high and low potency benzodiazapines, anticonvulsives, stimulants, antimanic agents, and even antipsychotics.

Even the more widely accepted assumption that treatments from the same theoretical model are functionally equivalent may not be supportable. Malik, Alimohamed, Holaway, and Beutler (2000) systematically compared three different renditions of Beck's cognitive therapy (CT), all conducted within the context of a randomized clinical trial study. One of the CT treatments followed the original Beck, Rush, Shaw, and Emery (1979) manual, and the sample was a group of older adults with major depression (Thompson, Gallagher, & Breckenridge, 1987; Thompson, Gallagher-Thompson, et al., 1991). The second manual was a group adaptation of CT (Yost, Beutler, Corbishley, & Allender, 1986) and was applied to middle-aged patients with major depression (Beutler, Engle, et al., 1991). The final manual was adapted for couples with alcohol abuse problems (Wakefield, Williams, Yost, & Patterson, 1996) and was applied to a sample of patients who had depression and chemical abuse problems (Beutler, Patterson, Jacob, Shoham, Yost, & Rohrbaugh, 1994; Karno, Beutler, & Harwood, in press). Therapists were all trained to criteria using the same compliance measure and all were supervised by experts in the CT approach through the use of audio or videotapes.

Malik et al. (2000) found that although CT had a common profile, generally, the three specific therapies were also significantly different from one another in several important ways. One therapy was less therapist guided than the others, one had less of a symptomatic focus, and one took a very different approach to patient emotions. This finding raises serious questions about the validity of collapsing therapies simply on the basis of their common theoretical origins or similar labels while ignoring differences that reflect particular settings, therapists, and populations.

WHAT IS COMMON AMONG PSYCHOTHERAPIES?

The dodo has pronounced that what the therapist does makes little difference to outcome and that widely different treatments all exert identical effects. But to what, then, can we attribute the positive effects of psychotherapy? Often in psychotherapy literature (e.g., Luborsky et al., this issue; Wampold, 2001), the therapeutic relationship is credited for the Dodo bird verdict, and it is implicitly assumed that relationship factors contribute the preponderance of variance to treatment outcomes. This logic has led some (e.g., Andrews, 2000) to portray relationship factors as responsible for upwards of 80% of observed variance in outcomes.

Ignoring the questionable assumption that therapy procedures do not contribute to the therapeutic relationship, research reveals that, in fact, the therapeutic relationship contributes little more to the independent prediction of outcome than does the unselective use of different therapeutic approaches. The effect size (described by correlations) attributable to relationship factors is quite modest, averaging in the mid .20s (Horvath & Symonds, 1991; Martin, Garske, & Davis, 2000). This translates to relationship factors producing no more than 10% of the variance in outcomes, approximately that attributable to the "specific effects" of psychotherapy (Shapiro & Shapiro, 1982; Luborsky et al., this issue; Wampold, 2001).

Thus, even if one is inclined to accept the existence of the dodo bird, the logical extension of this viewpoint does not permit investment in the salience and explanatory power of the therapeutic relationship. A better explanation is needed before we transform the dodo bird to a phoenix, under the label of "therapeutic alliance."

FINAL COMMENTS

Engage in a thought experiment: The Drugs-R-Us pharmaceutical company has developed a new drug. They call it Recessitation. They believe that it is a more effective treatment for pain than aspirin. Suppose that sugar pills alone will positively affect 60% of people (the placebo effect) and aspirin alone will positively affect 70% of people, a 10% gain. We do not know in advance which people will be affected by each of these two substances, however, and, unfortunately, aspirin is the only substance to which Recessitation will bind. Now, with such high rates of benefit from aspirin or sugar, how can we test the new medication, or better yet, should we?

Imagine that unbeknownst to us, Recessitation has a very specific and powerful effect among a subgroup of patients who have curly hair, eliminating pain in 90% of those patients, but has no effect on patients with straight hair, with or without the aspirin bond. Also imagine that, unbeknownst to us, straight-haired patients are the only ones who respond to sugar pills, and are 80% likely to benefit.

Following conventional procedures established by the (fictitious) National Institute of Drug Testing, we select what we believe to be a homogeneous group of 300 patients, based upon their shared diagnosis of chronic back pain syndrome, and randomly assign them to the Recessitation, aspirin, and sugar-placebo treatments. Suppose that there are, in our sample of 300 chronic back pain patients, only 60 who have curly hair, and, by chance, they are evenly distributed among the three groups—the miracle of randomization. What is the likely result of our experiment?

It is apparent that, under these conditions, the placebo group (patient factors?) will produce 64 responders, slightly better than usual because of the proportionately high numbers of curly-haired people. Aspirin (therapeutic relationship?) will produce 70 responders, a nonsignificant difference from placebo, and Recessitation (psychotherapy procedures?) will produce a dismal 18 responders, showing what will be interpreted as a suppression effect. In fact, the latter results are because Recessitation only supresses the effects of the placebo among straight-haired participants; it has enhanced effects among curly haired people.

With this result, Recessitation will never be marketed, even though if we could discover the undisclosed moderating effects, we would be able quite simply to successfully treat approximately 90% of patients who have chronic back pain syndrome by matching the treatment to the patient. This would represent a substantial gain over aspirin alone.

Our thought experiment is paralleled in the psychotherapy literature. Major depression, for example, has about a 70% response to any active treatment and a 60%+ response to placebo (Beutler, 1998; Kirsch & Sapirstein, 1998). But depression is also subject to relapse and can be extremely debilitating. As in our thought experiment, in psychotherapy research, the methodological desirability of reducing variance by selecting homogeneous samples has been translated into grouping patients who share a given diagnosis. But there is no evidence that reducing variance in the dependent variable by this procedure does in fact reduce variance in those variables that are important to differential treatment response. The degree of relevant homogeneity achieved by homogenous diagnostic groupings may be as mythical as Lewis Carroll's dodo bird.

Unbeknownst to those who are intent on raising the dodo bird from extinction, there are distinguishing qualities of patients within diagnostic groups that result in them being more and less responsive to different forms of treatment (e.g., Barber & Muenz, 1996; Beutler, Clarkin, & Bongar, 2000; Beutler, Engle, et al., 1991; Beutler, Goodrich, Fisher, & Williams, 1998; Beutler, Harwood, et al., in press; Beutler, Mohr, Grawe, Engle, MacDonald, 1991; Beutler, Moleiro, & Talebi, in press; Cooney et al., 1986). Like our fantasy experiment, the dimensions that form these subtypes are largely ignored, thereby resulting in a misattribution of what causes treatment effects.

In a recent study of patients with comorbid depression and chemical abuse problems, Beutler, Moleiro, Malik, and Harwood (2000) inspected the separate contributors to outcome of initial patient qualities, types of interventions, strength of the therapeutic relationship, and the overall fit of four patient and treatment variables. We determined that each of these groups of variables added to the prediction of treatment benefit. Patient qualities were important, as suggested by the literature on placebo effects. So were specific interventions, as suggested by research on therapy effects. So was the quality of the therapeutic relationship, supporting those who believe in the Dodo bird verdict. But fit of the treatment to the patient was important as well. While the different types of variables contributed different amounts to the prediction of outcome, depending on the particular aspect of improvement investigated, they collectively added to one another to achieve predictions that approached 90% efficiency in some analyses. Perhaps the dodo bird should remain extinct.

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